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JOURNAL

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OF THE

MASSACHUSETTS ASSOCIATION

OF

BOARDS OF HEALTH

An Official Quarterly Record of
Information for the Public

JULY, 1899

Proceedings of the April Quarterly Meeting

Subjects: The Typhoid Bacillus in the Urine—
Bacteriological Diagnosis of Diphtheria

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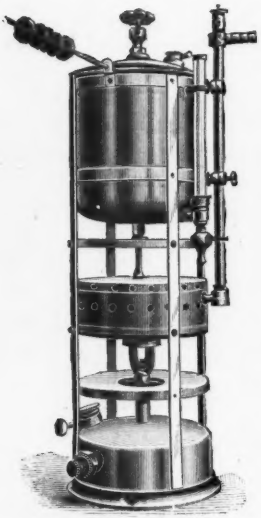

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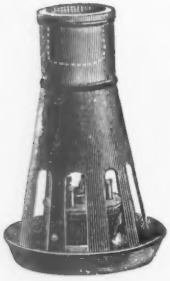

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All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

THE OFFICIAL JOURNAL OF THE ASSOCIATION is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them. No part of this matter is printed in any other periodical.

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All communications to the Association should be addressed to the Secretary, **EDWIN FARNHAM, M.D., City Hall, Cambridge, Mass.**

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JOURNAL OF THE

MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

ORGANIZED 1890.

[The Association as a body is not responsible for statements or opinions of any of its members.]

VOL. IX. July, 1899. No. 2.

APRIL MEETING

OF THE

Massachusetts Association of Boards of Health.

The April quarterly meeting of the Massachusetts Association of Boards of Health was held in Boston at the Parker House on the afternoon of Thursday, April 27, the President of the Association in the chair. Following is a report of proceedings: —

THE PRESIDENT.— The Secretary of the Association has been unexpectedly called away by death in his family, and I will ask you to nominate some one to temporarily take his place.

DR. CHASE.— Mr. President, I would nominate Dr. Frank P. Denny, of Brookline, for temporary Secretary.

The motion was seconded by Dr. Durgin, and adopted.

THE PRESIDENT.— The Executive Committee reports to the Association the names of the following candidates for election to membership: Dr. A. A. Etienne, a member of the Board of Health of Ware, Mass.; Dr. E. P. Pierce, of Springfield, Mass.; Dr. Atherton P. Mason, of Fitchburg; Dr. Manual V. Sylvia, of the Board of Health of New Bedford; and Dr. Thomas B. Smith, of Lowell. Is it your pleasure that these candidates be elected to membership in the

Association? If so, you will signify it by saying, Aye: contrary-minded, No; and they are so elected.

The reading of the records will necessarily be dispensed with, as the records are not here.

The miscellaneous business of the Association is now in order. I should like to ask if there are reports of any committees. Dr. Burr, I believe, has a report to make on behalf of a committee.

ANTI-RABIC INSTITUTIONS.

Dr. Burr.— Mr. Chairman, I believe I was appointed a committee of one to inquire and report at this meeting the number of anti-rabic institutions in this country and the fees for treatment. I sent out questions to the State Board of Health of each State in the Union, and also a number of the boards of health of Canada and Mexico. The questions were as follows: "Are there any anti-rabic institutions in the State?" "How many are private?" "How many are public?" and "The fees for treatment?" I received reports from all the States with the exception of some eight or nine. No reports were received from Alabama, Arkansas, California, Indiana, Mississippi, Nebraska, North Dakota, South Dakota, Texas, or West Virginia. Of all the reports received, only three States report anti-rabic institutions: one at the Pasteur Institute of New York, one in Baltimore, and one at Chicago. All three are private institutions. There are only three of them, so I will read the letter of each one.

"The only Pasteur Institute in Illinois is the one directed and founded by me." This is from Dr. Lagorio. "It is a private institution, and there are no State institutions of the kind. The fees for the complete Pasteur treatment are $100. Of course, many indigent poor have received the treatment gratis."

The Pasteur Institute of New York is that conducted by Dr. Gibier. He writes as follows: —

"In answer to your letter of the 28th of March, we beg to say that our regular fee for preventive treatment of hydrophobia is $200, including board and room for fifteen days. This is the amount paid to us by the State of New York for its indigent patients."

And there is one from Baltimore, as follows: —

"We have a very good Pasteur Institute here at the City Hospital, Saratoga and Calvert Streets, under the direction of Dr. N. G. Kierle." He does not give the fees.

The result is that there are three anti-rabic institutions in the country, all three being private institutions. The fees are from $100 to $200 for treatment.

THE PRESIDENT.— I should like to ask if it is not true, Doctor, that Dr. Biggs has lately made a provision in New York, on behalf of the city of New York, for the treatment of these cases.

DR. BURR.— I wrote to the New York Board of Health direct, because I had in mind that there was some sort of an institution connected with the Health Department proper; and I received the following letter: —

"Yours of the 28th inst. requesting information respecting institutions in the State of New York for the treatment of rabies is received; and in reply I beg to state that your communication has been forwarded to Dr. Paul Gibier, No. 313 West Twenty-third Street, who will doubtless be able to give you the information desired."

THE PRESIDENT.— I asked the question because Dr. Theobald Smith told me the other day that in a recent conversation with Dr. Biggs at the institute he was informed that they did intend to undertake the treatment of people suffering from supposed rabies or liable to the infection. If they do, it would be a great assistance, because it would probably be the most thoroughly conducted treatment of the sort in this country.

DR. BURR.— In one letter which was received from Baltimore it was stated that in New York they had been using the virus made by Dr. Kierle of Baltimore; that is, the Municipal Laboratory in New York were using the virus made by Dr. Kierle of Baltimore, although in the letter received from New York Board of Health nothing is said in regard to it.

THE PRESIDENT.— Has any other committee anything to report? Dr. Durgin?

THE INSPECTION OF MILK.

DR. DURGIN.— As chairman of the Committee on Legislation, I have very little to report. I would say that the last time the committee made any appearance at the legislature was about a year ago, and then in behalf of what was proposed in the Association, that the appointment of milk inspectors should be transferred to the boards of health. We were badly whipped. There seemed to be some great fear in the committee as to what all this meant; and I can assure you that, while the proposition made there seemed to us extremely simple and honest, the appearance of the committee did not indicate to me that they felt so. Those whom we had taken into our counsels and thought they were with us were there in force against us, and with them leaders from some of the inland cities and towns. I think it was a mayor who put in a very ponderous appearance in a political and very boisterous way, and accused the boards of health of being what they should not be. He made much of the fact that in his city, I think it was Lowell —

A MEMBER.— Guilty.

DR. DURGIN.— He had vigorously exercised his prerogatives over his board of health, and — At any rate, we were glad to retire whole. The Boston Board of Health, after this, having sufficient legislation for its own use, put in force those regulations which you all approved. We trimmed them a trifle, and adopted them last December. We have put these regulations in force in the interest of producers and consumers, and hope to see the farmer better paid and the consumer served with cleaner and safer milk. I have had some of the regulations and blanks brought over, and perhaps you would like to take them home with you. We have the assurances now of the milk contractors, whose interests are identical with ours and those of the consumers, that they will co-operate with us; and I have no doubt that we shall get on harmoniously. I hope that the other boards of health will soon find it possible to follow suit, for there cannot be a much more profitable line of work than in the care of the milk supply.

THE PRESIDENT.— Is there anything else under the heading of Miscellaneous Business to come before the Association at this time?

DR. BIGELOW.— Mr. President, I don't know but it is a little too soon to begin to speak about another feed, when we are all theoretically full.

THE PRESIDENT.— I think the Association will pardon the allusion.

DR. BIGELOW.— But there is a little town in Massachusetts, a few miles out of Worcester and not far from Boston, which goes by the name of Leominster. The Board of Health of Leominster would like to put in an application for the State Board and the local boards to meet with us in October for the quarterly meeting and for a feed. We have no sail down the harbor, we have no Parker House; but we will try to give you a good feed and show you what attractions there are in that vicinity. I hope that this will be received by the Executive Committee and the members willingly, and that the Association will vote to have the October meeting in Leominster with the local board of health of that town.

THE PRESIDENT.— I understand that there has been another invitation informally extended to the Association for this meeting; and I presume the usual course will be followed if there be no objection, and that the Executive Committee will report to the next meeting of this Association their own feelings on that subject. If there is no objection, it will be understood that the invitation is referred to the Executive Committee for the purpose of report to this Association at its July meeting. Is there anything else under this heading, Miscellaneous Business?

ANTI-RABIC INSTITUTION FOR MASSACHUSETTS.

DR. GAGE.— Might I interrupt a moment to ask Dr. Walcott if the State Board of Health of Massachusetts have taken any action toward establishing an anti-rabic institute in this State, or whether it is their intention and purpose,— if I am not getting ahead of time?

THE PRESIDENT.— No, the State Board of Health in that matter holds itself entirely at the disposal of the local authorities of Massachusetts; and, if there appeared at any time to be a sufficient justification for the expenditure of money in that direction, we should be very glad, indeed, to go before the legislature and ask for the appropriation. It was somewhat with reference to this matter that Dr. Smith, the pathologist of the State Board of Health, has been making a recent examination of some of the more prominent institutions in the way of public work in the field of preventive medicine.

DR. GAGE.— May I interrupt again? If it is not out of order, I should like to make a motion in reference to this question. I should like to move that it is the sentiment of this Association that it is necessary and expedient that such an institute be established in this State by the State Board of Health.

DR. DURGIN.— I second the motion.

THE PRESIDENT.— You have heard the motion of Dr. Gage. Is there anything to be said by any member of this Association upon that subject? It is moved and seconded that it is expedient for the State, through its State Board of Health, to undertake the care of persons infected by rabies or supposed to be infected by rabies. If that be your pleasure, you will signify it by saying, Aye; contrary-minded, No. It is so voted.

If there is no other business, we will then proceed to the regular programme for this afternoon's exercises. The first paper is a paper on "The Presence of the Typhoid Bacillus in the Urine, and its Relation to the Public Health," by Mark W. Richardson, M.D. I have the pleasure of introducing to you Dr. Richardson.

ON THE PRESENCE OF THE TYPHOID BACILLUS IN THE URINE, AND ITS RELATION TO THE PUBLIC HEALTH.

BY MARK WYMAN RICHARDSON, M.D., BOSTON, MASS.

It is the purpose of this paper to present to this Association briefly the results of bacteriological investigations which the writer has made upon the urines of persons sick with typhoid fever. These investigations have been made in the wards and in the pathological laboratory of the Massachusetts General Hospital, and have required for their completion about a year and a half. Accounts of the work have been published in the *Journal of Experimental Medicine*, 1898, No. 3, and 1899, No. 1. Any one desiring details of the investigations may seek them in this journal.

To take up the most important literature briefly: —

Neumann (1890) examined the urines in forty-eight cases of typhoid fever, and found bacilli in eleven instances. The bacilli were always present in large numbers, and in some cases persisted far into convalescence.

Wright (1895) obtained bacilli in six out of seven cases.

Besson (1897) found typhoid organisms in six out of thirty-three cases.

Horton-Smith (1897) examined the urines of seven cases, with three positive results. Horton-Smith also pointed out the fact that the bacilli can be demonstrated, oftentimes, in cover-glass preparations from the urine, and, further, that the organisms may be so numerous as to render the urine distinctly turbid.

Petruschky (1898) obtained three positive results in fifty cases. The bacilli remained in one case two months after the beginning of convalescence; in a second case, six weeks; and, in a third case, eight days after the temperature had become normal. Petruschky estimated that in one case a cubic centimeter of urine contained 170,000,000 bacilli.

Cushing has seen a case at the Johns Hopkins Hospital in which five years after typhoid fever the urine showed evidence of marked cystitis and typhoid bacilli in pure culture.

The cases investigated by the writer number one hundred and four, with twenty-three positive results. The conclusions reached agree entirely with those of the investigators just enumerated.

1. Typhoid organisms are present in the urine of about 25 per cent. of the cases.

2. The number of the bacilli is often enormous. Indeed, the urine may be distinctly cloudy simply from the organisms present.

3. The bacilli may persist in the urine for weeks, months, or even years after convalescence begins.

4. The virulence of the bacilli obtained from the urine was tested in one instance, and their pathogenic power equalled in every respect that of cultures obtained from the internal organs and the stools.

5. The urines of typhoid patients should, therefore, not only be rigorously disinfected during the disease, but should also be supervised carefully during convalescence.

Studies upon methods for remedying this condition resulted as follows: —

1. Irrigation of the bladder with boric acid (5 per cent.) was of no avail. Corrosive sublimate (1.7000) was effective.

2. Salol by the mouth, ten grains t, i, d, was effective in a single case. In two cases the bacilli persisted in undiminished numbers.

3. Urotropin was used in nine cases in a dosage of ten grains three times daily, and with excellent, not to say extraordinary, results. In every instance the organisms were removed. Moreover, this happy result was obtained oftentimes in twenty-four to thirty-six hours; and a urine which had been cloudy with bacteria would in two days become absolutely clear. In two cases, when the urotropin was stopped, the bacteria returned to the urine; but a second or third dose of the drug sufficed to remove them entirely. In this connection I will read a short note which I received recently from Horton-Smith, who, as already stated, has worked upon this same subject at St. Bartholomew's Hospital in London, and to whom I had written of the virtues of urotropin. He writes: —

Dear Doctor,— I am just sending this to say that we have just tried urotropin in a suitable case. The typhoid bacilli were present in enormous quantities, and

ten grains of urotropin were given three times a day for two days. At the end of that time the urine was sterile. Very many thanks for letting me know of the drug.

We have, then, before us a condition, and a remedy for that condition,— a condition the importance of which was recognized nearly ten years ago, but which has never received the attention which it has deserved. In fact, it has been my experience that the urines in typhoid fever have been utterly neglected, while the stools were rigorously disinfected. In my opinion, however, the urines are much more to be feared than the stools. To be sure, only 25 per cent. of the cases show infected urines; but the enormous numbers of the bacteria, and their persistence into convalescence, make them extremely dangerous, not to the patients themselves,— for they are, of course, immune,— but to those in their environment.

We keep our diphtheria patients in quarantine until their throats are free from Klebs-Löffler bacilli. Is it not just as important that typhoid convalescents should be subjected to equally strict supervision? The answer to this question must be in the affirmative.

How shall this supervision be carried out?

In the first place the urines as well as the fæces must be carefully disinfected throughout the disease. Then, as the temperature reaches the normal point, the urine should be examined bacteriologically, to determine the presence or absence of bacilli. If no bacilli are found, then, of course, no active measures are indicated. Before the patient is allowed to go abroad, however, — that is to say, after from two to three weeks,— a second and final examination should be made.

If the urine at the beginning of convalescence shows typhoid bacilli, then urotropin should be given in ten grain doses three times daily for a week. A week after the administration of the urotropin has been stopped, the urine should be examined again. If there are, at that time, no bacilli present, the patient may be released from supervision.

Finally, it seems to me more than probable that during the coming summer and fall we shall see an unusual number of typhoid cases. The reason for this will lie in the fact that so many soldiers convalescent from typhoid have returned from Cuba and Porto Rico.

These soldiers will be responsible in part because of typhoid organisms persisting in the stools; but the danger of this method of infection will, in my opinion, be surpassed a thousand-fold by that of the 25 per cent. of typhoid urines which, entirely unsuspected, have been deposited broadcast through the camps and the country in general.

I have here some typhoid urine which very fortunately turned up at the hospital this morning, and which shows the cloudiness which these typhoid urines may often show, very largely from the presence of bacilli alone. In this there is a certain amount of sediment, which of course accounts for part of the cloudiness; but most of it, I am sure, is due to the bacilli alone. Also, I have a couple of plates here which will demonstrate very forcibly, I think, the numbers of these bacilli. This first plate represents the number of bacilli in a single platinum loop of urine,— that is to say, the ordinary bacteriological platinum loop, which is, of course, a very small amount; and yet you can see, by holding it in the proper light, that in the plate there must be thousands of colonies, each colony, of course, representing the growth from a single bacillus. The second plate represents approximately a cubic centimeter of urine, and you will see that the bacilli were so numerous that the individual colonies cannot be made out at all.

THE PRESIDENT.— You have heard this most interesting communication of Dr. Richardson's. Is there any discussion upon the matter, or are there any questions to be asked, which, I presume, Dr. Richardson will very gladly answer?

DR. BROWN.— Mr. Chairman, I am quite unfamiliar with the drug mentioned by the doctor. I should like to know if it has any other effect than the destruction of the bacilli in the bladder, if there are any other therapeutical indications, or if it accomplishes any other result, and if there are any contra-indications to its use.

DR. RICHARDSON.— Urotropin is a comparatively new drug, and was first got out by Nicolaier in Göttingen. Nicolaier first claimed that it was an especially good drug for the solution of uric acid calculi, but this claim has not been verified by other observers. But, although it has not been able to dissolve calculi, it has been found to be a very strong urinary antiseptic, and has been used very largely in

cases of cystitis, especially of those due to the colon bacillus and its varieties. On cystitis due to tuberculosis or to gonorrhœa it is said not to have much effect. Urotropin is said to be a combination of formaldehyde and ammonia, and it is excreted in the urine partly as urotropin and partly as active formalin.

DR. BIGELOW.— Mr. President, I should like to ask a question. I should like to ask if this drug, urotropin, is supposed to modify or to cut short the progress of the disease by destroying the bacillus in the urine. If given in the earlier stage of the disease, will it cut short the progress of the typhoid fever?

DR. RICHARDSON.— I don't think that it has ever been tried in that way. There is certainly no literature on the subject.

DR. MILLER.— Mr. President, I think this is one of the most interesting papers we have had in a long time. It takes up this subject in a new light. While for years those of us that have treated typhoid fever have been very particular that the stools be disinfected and disposed of, the mass of us have never thought to be particular about the urine. As the doctor has just suggested, typhoid fever patients that have the urine loaded with this bacillus may deposit the urine around almost anywhere; and probably that is one great cause of the spreading of typhoid fever. I, for one, am very glad that the doctor has taken up this subject. It opens it up in a new light. I hope that in some hospitals, where they have a chance to test it, this drug, urotropin, will be used in the early stages of typhoid fever, with a view of seeing if it will not shorten the disease. To me this is a very interesting subject, because we have in many parts of the State considerable typhoid fever.

DR. GAGE.— Mr. Chairman, I should like to ask Dr. Richardson what the urotropin is given for. Do the bacilli present in the urine always produce symptoms referable to the bladder of the nature of cystitis?

DR. RICHARDSON.— No. It is quite a curious fact that in these cases in which the urine may be loaded with bacilli it is comparatively rare that there is an actual cystitis.

DR. GAGE.— The practical question which comes home to us who have a case of typhoid fever is this: Are we going to give urotropin in every case of typhoid fever or are we not, and how are we going to

be able to give it? It is going to be difficult for us at home to know whether there are any bacilli present in the urine. I wonder what clinical evidence would be of use to the physician to determine the need of urotropin.

DR. RICHARDSON.— I think it is practically impossible to tell in every case whether or not a typhoid patient has bacilli in his urine without a bacteriological examination. Hence, unless such examination can be made, I think that all cases should be given the urotropin for a week; and, in that way, I feel sure that certainly ninety-nine cases out of a hundred would be relieved of their dangerous character.

DR. MILLER.— Mr. Chairman, I should like to ask the doctor one question, with your permission,— whether those cases of typhoid fever where this bacillus has been found abundantly are more severe than the cases where none are found.

DR. RICHARDSON.— Yes, I think that much the larger percentage of infected urines occur in the more severe cases; that the comparatively light cases are those which will show a smaller percentage of urine infected by bacillus. I should like to say also that I think the cases of epididymitis and orchitis — which occur, rarely, to be sure, in typhoid — are ascending infections from the urine. Certainly, the two cases of epididymitis which I have seen in the Massachusetts General Hospital in the last two years have been cases in which motile bacilli were present in the urine. In the first case, it was the typhoid bacillus: in the second case, it was a motile variety of the colon bacillus.

DR. BROWN.— Mr. Chairman, there is one more question I should like to ask, and that is whether this drug disinfects the intestinal canal.

DR. RICHARDSON.— That I cannot say. I don't think that has ever been thoroughly worked out.

DR. DURGIN.— It strikes me that the practical utilization of this information would be to require a bacteriological examination in a typhoid case before discharge as much as in the case of diphtheria. We used to discharge our cases of diphtheria when they appeared to the naked eye to be free from the disease, but we found it to be a mistake; and we now require before discharging from isolation that there shall be a bacteriological examination which will reveal the fact

that the diphtheria bacilli have gone. Now, if we are to utilize the facts given us to-day, we must require a bacteriological examination of the urine of the typhoid patient, to show that the presence of the typhoid bacilli have gone, before the patient can safely be discharged from treatment or excused from the disinfection of his urine.

DR. CHAPIN (of Springfield).— I should like to ask, Mr. President, how long bacilli are present in the stool, whether or not it would be necessary to make daily or weekly examinations of the stools of the patients before discharging them, or of the milk, or of the saliva.

DR. DURGIN.— Well, we have to take up one thing at a time. We are hitting the urine now. If we should find that the bacilli are not gone from the fæces after the person appears to be well, we ought to apply the test there.

DR. RICHARDSON.— Mr. Chairman, I should like to say that what work has been done upon the fæces has tended to show that the bacilli disappear from the fæces very quickly after the temperature reaches the normal point. As far as our present knowledge goes, therefore, the stools of convalescent typhoids are little to be feared, when compared with the urines.

A MEMBER.— I should like to inquire for the technique of the examination.

DR. RICHARDSON.— My method of procedure was as follows. In the case of male patients the glans penis was thoroughly scrubbed with corrosive sublimate (1.1000), and then the patient was told to pass his water in two portions; and only the second portion of urine was used for bacteriological examinations. Female patients were catheterized with all antiseptic precautions. The urine should, of course, be passed into a sterile vessel of some sort. Then two agar plates were made, one consisting of a single platinum loop of urine and another of approximately a cubic centimeter; and I found these two plates sufficient for my purpose.

DR. CONNELL.— I should like to ask the doctor how soon in the course of the disease the bacteria appear in the urine.

DR. RICHARDSON.— They do not appear, as a rule, until the later stages of the disease, generally about the third week. I think that the large number of organisms in the urine is not due to the excre-

tion as such, of course, of so many bacilli. I think it is due rather to the excretion of a few bacilli by the kidneys, and then to their multiplication in the bladder.

DR. HILL.— I should like to ask Dr. Richardson what confirmatory tests he used.

DR. RICHARDSON.— In testing the typhoid cultures which were isolated, the following methods were used: —

1. Bouillon for size, shape, and motility = motile.
2. Sugar agar for gas production = no gas.
3. Potato — visible or invisible growth = invisible growth.
4. Gelatine — presence or absence of liquefaction = no liquefaction.
5. Peptone solution — production of indol = no indol.
6. Litmus milk — acid production and coagulation = sl. acid production no coagulation.
7. Reaction to typhoid serum = positive in every case.

DR. CHAPIN (of Providence).— I did not quite understand from Dr. Richardson whether there were any contra-indications at all to the use of urotropin, and whether it can be continued for quite a while without inducing any symptoms, or whether there are any other physiological actions of the drug.

DR. RICHARDSON.— As far as I have been able to learn from the literature and from my own experience, there are no bad effects from urotropin. A great many of these cases of typhoid showed a certain amount of albumen in the urine, and also casts; but I never could see that the amount of albumen or that the number of casts was increased. That has been the experience of other observers also. The dose as generally given is ten grains three times a day, but the drug has been given up to eighty or ninety grains a day. With large doses there has been some complaint of burning at the neck of the bladder, and perhaps the appearance of a few blood globules in the urine; but, with this exception, I have never seen any record of any bad effects from the drug.

DR. BRYANT.— I should like to ask if you have noticed any difference in this use in acid or alkaline urine.

DR. RICHARDSON.— The opinions are rather at variance on that point. Some say it works better in an acid urine, others say it

works better in an alkaline urine. Certainly, in most of those cases of typhoid, the urine has been acid. On the other hand, urotropin is said to be especially good in the cases of cystitis due to ammoniacal fermentation. It would seem, therefore, that the reaction was not especially important.

THE PRESIDENT.— Is there anything else to be said upon this most interesting subject before we pass to the next paper upon the programme? I certainly desire, in the name of the Association, to thank Dr. Richardson for this most interesting contribution to our meeting [applause].

The next is a paper upon "Some Questions relating to the Bacteriological Diagnosis of Diphtheria," by Dr. Hill, whom I now introduce to you.

SOME QUESTIONS RELATING TO THE BACTERIOLOGICAL DIAGNOSIS OF DIPHTHERIA.

FROM THE BACTERIOLOGICAL LABORATORY OF THE BOSTON BOARD OF HEALTH.

BY HIBBERT WINSLOW HILL, M.D., DIRECTOR.

Mr. President and Gentlemen,— During a meeting of this Association at Gallop's Island, last summer, the desire was expressed by some of the members present for a fuller account of certain features of the diphtheria diagnostic work in Boston, such as could only be obtained from records reaching over a considerable period. That desire I have endeavored to meet in this article. There is, perhaps, little that is new or startling in the figures I have to submit; but they are of interest as indicating the actual possibilities and limitations of this work, when carried out for large communities and under modern conditions.

The data, other than the bacteriological results proper, from which these conclusions were drawn were supplied to us by physicians in the form of returns made on the cards which accompany the diphtheria outfits. It will not be surprising to you to learn that these

returns are often incomplete, and that some of them at least are inaccurate. Nevertheless, they furnish information which it would be difficult or impossible to obtain in any other way. Our cards read as follows: —

FORM 4. [*To be filled out by the Bacteriologist only.*]

BOSTON HEALTH DEPARTMENT.

Diphtheria: Case No. ..... D ..... Received .....

.....

.....

[*To be filled out by Physician only: see other side also.*]

Date of Culture ..... Hour ..... A.M. ..... P.M. For Diagnosis? ..... For Release? .....

Patient's Name ..... Age ..... Sex .....

Patient's Address .....

Date of Earliest Symptoms ..... Source of Infection .....

On the reverse the card reads as follows: —

Membrane present? ..... Nares? ..... Pharynx? ..... Tonsils? ..... Larynx? .....

If no membrane exists, state site of inflammation .....

Constitutional Symptoms .....

Clinical Diagnosis .....

Remarks .....

Physician's Name .....

Physician's Address .....

[*Please fill out both sides of this Card as indicated, for every Culture made.*]

It will be seen that, carefully filled out, such a card supplies us with a great deal of useful information. Fortunately, a sufficient number are so filled out as to make the statistics compiled from them useful, although far from perfect. In illustration of some of our difficulties, I may be pardoned for referring to a few of the more common omissions and mistakes.

The date of earliest symptoms, for instance, is often left out. Sometimes the date, when given, is not the same on any two of a series of cards coming from the same physician and relating to the same patient. The source of infection is hard to fix in most cases, so that omissions there can be readily forgiven. One or two cases, however, in which the clinical diagnosis was entered as diphtheria gave also the source of infection as "a case of tonsillitis next door." It is hard to understand quite what this means. Perhaps the physician intended to imply that the diagnosis of tonsillitis made on the next-door patient was at fault.

The most serious source of trouble, so far as the executive work of the laboratory is concerned, preventing especially the prompt report to the physician which is so necessary and desirable, lies in the omission by the physician of his own name and address or of the patient's name and address. This is, of course, a clear case of oversight on the physician's part; and one can quite understand how, in the rush of practice, it may sometimes occur. But it is none the less apt to cause delay and trouble. One physician who requested urgently a prompt report on his culture omitted his own name and address, and also that of the patient, while giving otherwise a full account of the case. The report has now been waiting for him about six months, during which time he has perhaps treasured up against the laboratory some hard feelings, quite naturally, but of course, also, quite wrongfully. We have no clew to the identity of physician or patient under such circumstances, and cannot report.

By confusing the blank spaces left for the physician's name with that left for the patient, one big, heavily bearded man described himself on our records as a "female, aged seven years"! A good many cards come in without any designation as to whether the cultures they accompany were taken for diagnosis or for release. If the date of earliest symptoms be omitted also in such cases, a good deal of unnecessary trouble may be caused. I do not want to give the impression that the majority of physicians are careless about this matter of furnishing data. Many men are very careful and very conscientious about it. But there certainly are a few men in every city and town who do not stop to consider how important is all of the information asked for or how absolutely essential at

least some of it is. A carefully filled out card is one step in scientific advance,— a small one, certainly; but every little helps. If the bacteriologist is able to make every culture he receives tell a little in rendering diagnostic work better, more exact, and more intelligible, it reacts to the advantage of the public and of the profession, so that, in attempting to impress the importance of this feature of the diagnostic service upon the profession, I am but attempting to provide against one of those sources of error which retard progress, and retard it unnecessarily.

After this explanation the results we have obtained during the last nine months will be understood as representing an approximation to the actual facts,— an approximation only, it is true, but nevertheless an approximation of far more value than mere guess-work or the "general impressions" which one may form without the aid of fairly correct figures.

Turning now to our records, it is generally conceded that a positive culture for diagnosis is practically absolute evidence of the presence of diphtheria bacilli in the lesion from which the culture came, while a negative culture for diagnosis is not such good evidence of their absence. In a former paper the *a priori* reasons for this proposition were given. I shall now present the actual figures: —

TABLE I.

Total cases for diagnosis: bacteriological diagnosis, positive .	336	100%
Bacteriological diagnosis, positive on first culture . . .	312	92.5
Bacteriological diagnosis: negative on first culture; positive on following cultures	19	5.6
Bacteriological diagnosis: unsatisfactory on first culture; positive on following cultures	5	1.9

It will be seen that, out of all the cases for diagnosis which proved positive finally, the bacilli were found in 92.5 per cent. with one culture. Of these 336 total positive cases, the bacilli were probably present at the time of taking the first culture in most, and certainly were present later in all; but, nevertheless, about 5.6 yielded negative cultures from the first tubes taken. Those yielding "no growths" do not furnish any information to the physician; but the

negative cultures, about 5 per cent. of the whole, yield actually misleading information. This table then emphasizes once more the fact that a positive culture is reliable evidence of the presence of bacilli, a negative culture less reliable evidence of their absence. So far as these tables may be depended upon, about one case of diphtheria in twenty will yield a negative culture early in the attack. The natural conclusion is, then, that, if the physician has good clinical evidence that a case is really diphtheria, he should not readily reverse his diagnosis on the receipt of one negative culture. Two or three negative cultures should be obtained first.

It is also, I think, well known that a negative culture for release is less valuable than a negative culture for diagnosis. The reasons for this also have been already pointed out before this Association.* The following table, when compared with that already given, brings out this point clearly: —

TABLE II.

A.

Cases released on two or more consecutive negatives:	
Total cases	415
Showing positive cultures at some time or other	257
Reported as diphtheria, without bacteriological diagnosis, but subsequently released by two negatives	158

B.

Cases showing premature negatives:		
Total positive cases released	257	100%
Positive cases showing premature negatives	74	28.6
Positive cases not showing premature negatives	183	71.4

In Table A the total cases released by us on two consecutive negative cultures are given. Some of these yielded positive cultures for diagnosis in the first place, then ran their course, and were released by negative cultures. Others, however, had no cultures for diagnosis, but were reported as diphtheria on purely clinical evidence, and did not pass through the bacteriological tests until they came up for release. Of these latter, some showed positive cultures during the release tests, others did not. In Table A those cases which showed

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positive cultures at any time during their course, and were therefore positively identified as cases in which the bacilli were present, have been separated from those which showed negative cultures only, since the latter cases cannot definitely be known as diphtheria at all. They or some of them may have been diphtheria, but there is no bacteriological evidence on the point. In Table B, therefore, I have taken only the cases known to have shown diphtheria bacilli. Of these 257 cases, 74 showed negative cultures, followed by positive cultures. In other words, 28.6 per cent. of the positive cases showed negative cultures for release during the time the diphtheria bacilli were really present. From this the point is established that a negative culture for release is not as reliable an evidence as a negative culture for diagnosis, since a negative culture for diagnosis occurs in only 5 to 10 per cent. of cases where diphtheria bacilli are present, while a negative culture for release occurs in a percentage three to five times as great, under similar circumstances.

The relation of clinical to bacteriological diagnosis is a subject also of some interest. It has been claimed again and again, since bacteriological diagnosis was first instituted in various parts of the civilized world, that bacteriologists have discovered diphtheria bacilli in many cases which were clinically innocent, thus raising the total number of diphtheria cases now reported as such by adding in cases which were not, under solely clinical tests, classed as diphtheria at all. This is a favorite argument of the opponents of the use of antitoxin, who maintain that the absolute deaths from diphtheria are now as large, if not larger, than ever, while the admitted reduction in the *percentage* of fatal diphtheria cases these objectors explain as due, not to a lessening in the number of deaths, but to an increase in the total number of cases, the increase consisting largely of the innocent cases discovered, quite needlessly, by the bacteriologists. Such objectors cannot have considered the records on these subjects carefully. As a matter of fact, from the records of three laboratories where the results have been tabulated to show a comparison of clinical and bacteriological diagnoses, quite the contrary is true; for the bacteriological examination is found to really reduce the total number of cases instead of increasing them. In other words, physicians make a clinical diagnosis of diphtheria in innocent

cases more often than a clinical diagnosis of "not diphtheria" in cases really diphtheria, so far at least as laboratory records show. I have prepared a number of tables bringing out these points as follows. The laboratories from which the records come are those of the Pittsburg Board of Health for one year, of the Massachusetts State Board of Health for two years, and of the Boston Board of Health for nine months.

TABLE III.

PITTSBURG BOARD OF HEALTH REPORT, 1897.

For Year 1897.

Whole number of cases	897	
Clinical diagnosis, positive:		
Total cases	561	100%
Bacteriological diagnosis, positive	390	70
Bacteriological diagnosis, negative	171	30
Clinical diagnosis, negative:		
Total cases	336	100%
Bacteriological diagnosis, negative	274	81.5
Bacteriological diagnosis, positive	62	18.5
Actual concordant cases	74%	
Theoretical concordance	75.7	

The clinical diagnosis was negative in 37.3% of all cases where clinical diagnosis was given.

MASSACHUSETTS STATE BOARD OF HEALTH REPORT, 1897.

For Year ending March 31, 1897.

Whole number of cases	489	
Clinical diagnosis, positive:		
Total cases	354	100%
Bacteriological diagnosis, positive	239	67.5
Bacteriological diagnosis, negative	115	32.5
Clinical diagnosis, negative:		
Total cases	135	100%
Bacteriological diagnosis, negative	108	80
Bacteriological diagnosis, positive	27	20
Actual concordance	71%	
Theoretical concordance	73.7	

The clinical diagnosis was negative in 27.2% of all cases where clinical diagnosis was given.

For Year ending March 31, 1898.

Whole number of cases	727	
Clinical diagnosis, positive:		
Total cases	494	100%
Bacteriological diagnosis, positive	338	69.2
Bacteriological diagnosis, negative	156	30.8
Clinical diagnosis, negative:		
Total cases	233	100%
Bacteriological diagnosis, negative	176	75.5
Bacteriological diagnosis, positive	57	24.5
Actual concordance	70.7%	
Theoretical concordance	72.3	

The clinical diagnosis was negative in 32.0% of all cases where clinical diagnosis was given.

BOSTON BOARD OF HEALTH REPORT OF 1898.

Nine Months ending Jan. 31, 1899.

Whole number of cases	765	
Clinical diagnosis, positive:		
Total cases	219	100%
Bacteriological diagnosis, positive	124	56.7
Bacteriological diagnosis, negative	95	43.3
Clinical diagnosis, negative:		
Total cases	546	100%
Bacteriological diagnosis, negative	488	89.6
Bacteriological diagnosis, positive	58	10.4
Actual concordance	80%	
Theoretical concordance	73.1	

The clinical diagnosis was negative in 71.4% of all cases where clinical diagnosis was given.

The theoretical concordance given above is the concordance estimated from the results tabulated which would be obtained as the actual concordance, had the number of positive clinical diagnoses equalled the number of negative clinical diagnoses.

SUMMARY.

Total cases reported as diphtheria on bacteriological diagnosis, but clinically "not diphtheria" at time of taking culture	204
Total cases reported as "not diphtheria" on bacteriological diagnosis, but clinically diphtheria at time of taking culture	537

In other words, the bacteriologist added 204 cases to the total, but at the same time subtracted more than twice as many. Thus for these three laboratories the full record stands thus: —

Total cases reported positive clinically and examined bacteriologically	1,628	100%
Total cases found positive bacteriologically	1,295	79.5
Total reduction in diphtheria cases due to bacteriological examination, so far as can be calculated from comparison of clinical and bacteriological diagnoses	333	20.5%

Hence it will be seen that a little over 20 per cent. of the cases which physicians set down distinctly and positively as diphtheria, from the clinical symptoms, were shown not to be diphtheria by the bacteriological diagnosis in these three laboratories. It must be remembered, of course, that this does not represent all the cases examined bacteriologically by any means; for in a considerable number of cases a comparison of clinical and bacteriological examination cannot be made, because the clinical diagnosis is doubtful or "not given." Nor do all the cases examined bacteriologically form more than a small part of all the cases reported as diphtheria. Many reported cases throughout this country never pass through a bacteriological test at all, although in Boston itself the present regulations provide that every case, except those terminating fatally early in the disease, shall so pass at some time or other during its course. Nevertheless, judging from the cases which do go through such a test, it is fair to assume that, if every case reported as diphtheria by physicians were thus tested for diagnosis early in the attack, then the total number of diphtheria cases would be reduced, although probably in a smaller proportion. It is especially interesting to note that Boston physicians send in for examination an extremely large number of cases which they consider clinically innocent. Thus, while the records of the other laboratories under consideration show from 27 to

37 per cent. of negative clinical diagnoses, the Boston Board of Health shows no less than 71 per cent. In Boston the reduction in reported cases, due to bacteriological examination alone, is 40 per cent. of cases examined. If, however, for each of these laboratories calculations be made showing the concordance and discordance of clinical and bacteriological results, supposing the number of clinically negative and clinically positive cases to be equal, a remarkable uniformity of results is found to exist in these three laboratories. Thus it is found that about 73 to 75 per cent. of clinical diagnoses are confirmed, about 25 to 27 per cent. are reversed, in each.

The relation of bacteriological results to the presence or absence of membrane in cases for diagnosis is another point over which a good deal of discussion has occurred. It has not infrequently been held that the bacteriologist often finds bacilli present, not only when clinical symptoms point to innocence, but even in the absence of membrane or anything that might be mistaken for membrane. The following table will answer this question : —

TABLE IV.

Bacteriological Diagnosis.	*Positive.*	*Negative.*	*Totals.*
Membrane present	252	588	840
Membrane absent	17	356	373
	269	944	1,213

Same table reproduced, in percentages : —

Bacteriological Diagnosis.	*Positive.*	*Negative.*
Membrane present	93.6%	62.3%
Membrane absent	6.4	37.7
	100%	100%

It will be seen that the 17 cases positive without membrane form less than 2 per cent. of the total 1,213 cases examined, and that more than 60 per cent. of the negative cases possessed a membrane or something like one. Of course, it is impossible to say how many of this 60 per cent. presented a real membrane and how many simply an exudate which was entered as membrane. More than 93 per cent. of the cases in which diphtheria bacilli were found showed membrane, however. The 17 cases in which no membrane existed

at the time of taking the culture had symptoms or other clinical reasons pointing to the possibility of infection.

The question of infection by exposure without any clinical symptoms or subsequent development of the disease which follows naturally here is rather a difficult one to answer definitely from our records, because of the small number of such cases sent in. Our records show 35 cases simply exposed to diphtheria, the cultures from which show 1 positive result, 33 negative, and 1 no growth. These were cases in ordinary private city practice. In institutions a different condition seems to hold good. For instance, in one home for children in Boston, wholesale cultures were made on the appearance of one or two cases. These cultures were repeated at frequent intervals. Only four positive cultures were obtained on the first examination, but a total of 40 positive cases were detected during the course of the whole investigation. I could not get exact records, but it would seem that many of these positive cases never developed clinical symptoms at all.

The length of time which bacilli persist in the throat is another subject often discussed. Here also the impression seems to exist that bacteriological examinations give trouble by prolonging unnecessarily a large number of cases for considerable periods of time. But a consideration of the figures obtained in practical work shows that this impression is mistaken. Thus the first final negative is obtained within twenty days from the date of earliest symptoms by us, in about 40 per cent. of cases which yield sufficiently complete data for tabulation. About 60 per cent. yield the first final negative before the twenty-fifth day. 74 per cent. of the cases show a first final negative within thirty days, about 86 per cent. within thirty-five days. Thirteen per cent. lasted from thirty-five to sixty-five days. We had no cases lasting longer than this. The average bacteriological history of an ordinary case is this: —

TABLE V.

	Average.
From earliest symptoms to first positive culture	2.5 days
From first positive culture to last positive culture	18.8 "
From last positive culture to first final negative	4.5 "
From first final negative to second final negative	2.7 "
Total	28.5 days

Thus the bacilli are found on an average for 21.3 days only from the earliest symptoms. Nevertheless, it would obviously be impossible to make an arbitrary time limit of, say, thirty days during which isolation should be enforced, and at the end of which release should be granted without bacteriological release; for such a time limit would prolong the isolation unnecessarily of about 40 per cent. of cases for ten days each, while, on the other hand, it would permit the release of about 26 per cent. of cases while still infective. Evidently, the release by bacteriological culture is then really not only a protection to the public health, but also a very considerable time-saver. Should twenty-eight days be selected as the arbitrary time limit, 40 per cent. of cases would receive at least five days' unnecessary isolation, whereas another 40 per cent. would be released while still infective.

To sum up the evidence so far available, it would appear: —

First.— That single cultures may be relied upon to settle the question of diagnosis in from 90 to 95 per cent. of cases.

Second.— In from 5 to 10 per cent. of cases a single negative culture for diagnosis is unreliable, so that, if the clinical evidence points to the presence of diphtheria, the diagnosis of diphtheria should not be given up unless two negatives at least are obtained.

Third.— In about 28 to 30 per cent. of positive cases one negative culture for release may be obtained while the bacilli are nevertheless still present; but two consecutive negative cultures for release will not allow more than 3 per cent. of such cases to escape detection.*

Fourth.— Bacteriological examinations tend: (1) to reduce the total number of cases finally diagnosed and reported as diphtheria, while making the actual diagnosis more correct; (2) to reduce the total time of isolation, while giving to each case the proper length of time, short or long, as may be necessary in each case.

Fifth.— The working error of bacteriological examinations for diagnosis is in positive *cases* about 5 to 10 per cent., if one culture be trusted to in each case. If two cultures be taken, the working error is practically nothing. In negative *cases* the working error also is practically nothing. Clinical diagnoses show a working error of from 10 to 40 per cent. Finally, while bacteriological diag-

* From records of the Boston City Hospital, where three negative cultures are required.

nosis is not perfect in its results, it affords the one definite method for approximating to accuracy and precision, it constitutes a court of appeal of great value, not only from a scientific standpoint, but also in the handling of obstructionist patients, both in private practice and in public health work, while, intelligently considered and carried out, it relieves those who employ it of a large share of responsibility in one of those diseases in which responsibility is particularly heavy, and clinical evidence often unreliable.

THE PRESIDENT.— Dr. Hill's paper is now before you for discussion or question.

DR. CHAPIN (of Providence).— Our experience in Providence in examining cases of diphtheria has, in most respects, been the same as that narrated by Dr. Hill; and in those points in which it differs I think there is a good explanation for the difference, perhaps. I should like to ask Dr. Hill, however, if I am mistaken in assuming that in Boston it is the custom to take a double culture in each case, one from the throat and one from the nose.

DR. HILL.— It is not at the present time, Dr. Chapin. The best we can do, apparently, in practical release work, is to insist on two negative cultures from whatever was the seat of lesion. We have not yet reached the point where we can enforce a regulation requiring release cultures from both nose and throat in every case, as we should. We recognize the sanitary value of the proceeding, and that it ought to be done; but, practically, it has never come to the point yet where we could do it.

DR. CHAPIN (of Providence).— Is it not the custom to do it in the primary culture?

DR. HILL.— No. In primary cultures the board of health has no control. The doctor need not take one at all unless he likes. When he does take it, he can take it from anywhere that he pleases.

DR. CHAPIN (of Providence).— I am very sorry, then, that my explanation of the difference in results is not what I imagined it was. In Providence our preliminary cultures are not so reliable as Dr. Hill states here. I cannot give you any of the figures now, because I carelessly forgot to bring a report with me; but, certainly, the first culture, even when the person is sick, cannot be trusted in 92 per

cent. of the cases. I imagined it was because it was customary in Boston to take two cultures, one from the nose and one from the throat, as I know it was some years ago, when the use of cultures was first introduced; and that has not been done in Providence, but a culture is made from the throat alone. I think, then, the unreliability of our first cultures must be due to the method of taking them, which is by means of a swab instead of a wire, as I believe you use here. I should dislike very much to think it was due to defects in the bacteriologist; and I can hardly think that it is so, because we have had several men examine the cultures in Providence, and they certainly all agree pretty well in regard to the ratio of unreliability of a single culture. We cannot get it right in 92 per cent. of the cases, as is done here.

In regard to the culture method decreasing the number of cases of diphtheria, it does not do that in Providence. It has increased the number of cases of diphtheria; and I think the reason for that is the very great efforts which have been made by the Health Department to secure cultures from every case of sore throat, no matter what it is. There are a great many physicians in Providence who, as a routine, take a culture from every case of sore throat they have. And we have made a very great effort, in looking up our cases, to look after those cases of tonsillitis next door that Dr. Hill tells about; and we are getting hold of all of the cases in the neighborhood, if possible, and have cultures taken from them. In a great many cases they do not show that the diphtheria bacillus is present. On the other hand, we often find a case of diphtheria in this way, a case which was spreading it perhaps in the school or elsewhere. I think that if every case of sore throat in Boston could be examined, or as large a proportion of cases as is done in Providence, you would have a great many more cases which could properly be called diphtheria. As a matter of fact, in Providence in 1896 the mortality of persons who were sick with diphtheria, and who did not have antitoxin, was 13.9 per cent.,— a decided reduction from the mortality of 27.41 of 1894, when there were neither antitoxin nor cultures, a reduction due not to the use of antitoxin, but to the use of the culture method of diagnosis.

In regard to the presence of the diphtheria bacilli in persons who have been exposed to the disease, and yet who have no membrane in the throat and who, perhaps, do not have any apparent inflammation of the throat at all, we have a large amount of experience, because for over two years we have insisted upon a bacteriological examination of the throat of each person in a family wherever there is a case of Klebs-Löffler diphtheria discovered, so that we have a record of many hundreds of cases of well persons whose throats have been examined,— persons who have been undoubtedly exposed to the disease, —and in a large proportion the diphtheria bacilli is found. In the two years 1897 and 1898 the proportion was 17.8 per cent. in adults, and 23.2 per cent. in children. The number examined was 1,522. We make it a practice in Providence, if possible,—and usually it is possible,— to maintain isolation in all such cases. Unfortunately, we have not been able to do what seems to me to be very important; and that is, to test the virulence of the bacilli in these cases. We have examined some of them, but not very many. My impression is that we would find that the bacilli in a considerable portion of the throats were not virulent. We have had just the same experience in institutions that Dr. Hill reports,— that, when diphtheria gets into an institution, and we can control all the patients and examine all throats and examine them repeatedly, we usually find a good many cases where the bacilli are present; and it often remains for some length of time, though it has been our experience in Providence that, if there are no clinical symptoms, the bacilli do not remain as long as when the clinical symptoms are present.

Another investigation which we have made is the examination of throats of persons who have not supposedly been recently exposed to diphtheria and who are perfectly well. We have had examined several hundred such cases within the last six or eight months, and in quite a number of them the bacilli were found present. In none of them that we have tested have the germs been shown to be virulent. But I will report one quite interesting case. Over a year ago, in an orphan asylum with a couple of hundred children, we had a diphtheria outbreak, and one death and several sick, and twenty-five or thirty cases of children not sick, but with the bacilli persisting

for some length of time. We got a negative culture from each person in the institution before removing the placard. About nine or ten months afterward I went there, and had a culture taken from every child in the institution. One of them showed the presence of the Klebs-Löffler bacillus. A short time afterward another one was reported as sick with the disease, with membrane upon the throat. The bacilli were found present in that case; and a culture was made from every other child in the institution, and no diphtheria bacilli were found to be present. What we have done in Providence leads us to think that the Klebs-Löffler bacillus as demonstrated upon the culture medium is extremely common in those that are exposed to the disease in the family, and that it is, also, not very rare in the community at large. Now a practical question comes. Shall we isolate every person in whom these bacilli are found? If not, how can we draw the line? If we are going to use the bacillus at all as a means of diagnosis for diphtheria, it seems to me the only logical thing for us to do is to isolate in every case where it is found.

Or there is another thing we can do. There are certainly a great many cases where the germ is not virulent when injected into guinea pigs. Now comes the second practical question. Shall we in every case examine the organism for virulence? That certainly would be very laborious. Or shall we rather test it in certain cases,— cases which have been running on an unduly long time or cases where it would produce very great hardship to maintain isolation? There is also another practical question which I should like to have Dr. Hill answer. In fact, I can ask him a great many questions. I should like to know if we are at all positive that there is any close relationship between the virulence of the organism as tested upon a guinea pig and its virulence when existing in the throat of a human being.

Dr. Hill.— I may be pardoned for correcting one misapprehension on the doctor's part. We have abandoned the platinum wire, and use swabs instead; and they appear to be very satisfactory. I have never had any experience with the platinum wire myself outside of the laboratory. I can only take as evidence what is told me by various physicians; but they seem to agree that it is very much more satisfactory to use the swab than the wire, so that I think that

that source of difference between our results will have to be ruled out.

With regard to the virulence of the organism found in the throat a good deal of work has been done. We have not done much of it in Boston: the question has never come up very strongly there. The Massachusetts State Board of Health did some very good work upon that point some two or three years ago, and so also have a number of other laboratories, so that it seems to be pretty well established that certainly 75 per cent., at least, of cases contain bacilli which are virulent even after membrane disappears. In Brooklyn, N.Y., up to 1898, under Wilson, who then had charge of the work, a time limit was set, after which every case was tested. If the bacilli lasted in the throat longer than four weeks, the bacilli were tested for virulence. His results were 77 per cent. "and over." I suppose the "over" refers to some cases in which they had laboratory accidents, etc.; but at least 77 per cent. gave virulence. Of course, in testing virulence there are a good many questions to be decided. In the first place, if a number of single colonies be tested, each of which represents a single bacillus in the throat, it will be found that some of those are virulent and some not at all, so that it is necessary in making the test to try a number of colonies rather than only one. That is, of course, a matter of technique.

Again, the dose which is given to the guinea pig is to be considered; and also on just what lesions the decision as to virulence is to be based is a very important question. Of course, if you expect the bacilli always to kill the guinea pig before you call them virulent, you may get a much lower rate of virulence than if you accept the formation of local lesion of a greater or less extent as sufficient evidence. In that way a good many discrepant results could be explained. But Dr. Theobald Smith's work in that line was very excellent, because he did not make the test by injecting the animals with cultures, but with toxin prepared from cultures; and he standardized this toxin in every case. In those cases which he completed, very little, practically no difference in virulence was observed in the bacilli obtained early in the disease and late in the disease. As to the relation of virulence in the human throat to virulence in the guinea

pig, it is a very difficult matter to settle. It is almost impossible to make direct inoculations, comparative inoculations, from one human patient to another, to compare with parallel guinea pig inoculations! If we could do that, we could settle that question very readily; but the clinical evidence of the infection from one case to another is usually so loose and so indefinite that it is usually impossible to base anything definite on it. It might be said just in this connection that Park's bacillus No. 8, which supplied large sections of this country for a long time with very virulent toxin for the manufacture of antitoxin, was isolated, I believe, from a particularly mild case.

With regard to the diphtheria occurring in institutions. Being in Canada within the last month or so for a day or two, the bacteriologist of the Provincial Board there told me he had been going through some institutions, taking cultures as the cases came in from every throat indiscriminately, and then at intervals taking cultures again from everybody in the institution. He found that a very few of those cases which came to the institution had the bacilli in their throats when they entered. The exact figures I have forgotten, but it was extremely small or practically nothing. On the other hand, although there were no diphtheria cases in the building, yet a few weeks after entrance the cultures taken from the inmates would show in a certain proportion of cases diphtheria bacilli, so that it seemed that in some cases, at least, institutions might become infected with bacilli in some way or other which very rarely produced typical clinical cases, while, nevertheless, the bacilli could be found on cultural examination. I think the question of institutional diphtheria will have to be discussed on an entirely different basis from the diphtheria of private families in private practice. I think that opinion is growing throughout the bacteriological world, and that we shall have to separate the statistics relating to institutions from those relating to private practice as well as from those obtained in hospitals. It is sometimes a little difficult to get the latter. Institutions in which an epidemic occurs may very often be in rather a rush, and may not feel like keeping careful records of every culture and of the condition of each patient, so that the bacteriologist is rather neglected after he sends in his report positive or negative, making it difficult to get definite data.

In all our records I have carefully excluded such institutions as we had, because what I wanted to demonstrate was the results to be obtained in actual house to house practice throughout the city of Boston, so far as we handle it.

DR. GAGE.— I should like to ask Dr. Hill if his records show in what percentage of the cases found to be positively diphtheria by culture antitoxin was used, Mr. Chairman.

DR. HILL.— No, sir, I cannot answer that question. We don't handle antitoxin at all: the State does it, so we have no record. However, I know that it is used in a very large proportion of cases. Physicians often come and ask for a diagnosis, so that they can influence the patients to let them use antitoxin where the patients object. In this way our diagnoses are often helpful.

DR. DURGIN.— I should like to say, in regard to the delay in the use of antitoxin for any purpose, that it is a very serious fault. The case is suspected of being diphtheria, the culture is taken, and delay is occasioned in getting the "positive" before antitoxin is given. It seems to me that we have advanced far enough in the use of antitoxin in the history of this dreaded disease, and especially as to its fatality without antitoxin, to warrant us in advising that just as soon as a suspicion is aroused of its being diphtheria the advantage given by antitoxin should be utilized. Cases come to my knowledge frequently where fatal results occur before antitoxin has been used and before the "positive" from the bacteriologist has been received. Inasmuch as we feel fairly certain that no injury will be produced by giving the antitoxin, it should be given whether you are certain that the suspicious case is going to turn out diphtheria or not.

DR. MILLER.— Mr. President, I should like to ask Dr. Durgin just one question. There are some cases in the country where we have follicular tonsillitis that is taken for diphtheria. In case antitoxin was used in those cases, what would be the effect? Would it be any advantage or any disadvantage? Suppose it was not diphtheria, but was supposed to be and it proved to be follicular tonsillitis, and antitoxin was used, would there be any particular effect upon the tonsillitis?

DR. DURGIN.— From all that we have learned concerning the effect of antitoxin, you will do your patient no injury whatever by giving antitoxin. You may save the patient's life by giving it early.

DR. CHAPIN (of Springfield).— The last case of diphtheria I had the bacteriologist declined to find any bacilli in after three examinations, and I was obliged to say that the patient never had diphtheria. It was as good a case of clinical diphtheria as I ever saw, with a firm patch on the posterior and lateral wall of the pharynx, increasing on the following morning to a small patch on the other side, yielding after the administration of antitoxin precisely as I have so many times seen true diphtheria membrane disappear after the use of antitoxin. I could not help feeling as if the antitoxin had treated the streptococcus which was working there very much as it would have treated the Klebs-Löffler. It may be all moonshine, but that is the way it looked.

DR. GAGE.— I did not know, Mr. President, that the question of the use of antitoxin was going to be raised. As long as Dr. Durgin has seen fit to speak of it and as long as here in Boston we never hear anybody — at least, I never have — raise any opposition to the use of antitoxin, I should like just to state a reply that I got from a respected member of our profession a short time ago, when I was holding a discussion with him in regard to the use of antitoxin. Perhaps Dr. Durgin will reply to him. We talked some time. He is a man who received his education long before the days of bacilli and antitoxin, has conducted a useful and successful and honorable practice, is a man who is mentally still awake; and yet I knew that he did not very often take a culture to the bacteriologist, and I was quite sure that he very seldom used antitoxin at all. In a little discussion with him about it I brought out this from him. This was his reply: No, he said, he did not very often use it. He said he was not convinced yet, he was not thoroughly convinced, that it was the thing to use. I asked him for his reason, and this was his reply: "You say that the deaths from diphtheria after the use of antitoxin are reduced from between 30 and 40 or even over 40 to perhaps 12 per cent. If that is true, then if you take the period since the use of antitoxin, say in the State of Massachusetts, for the five or six

years since it has been in use, you ought to find a very marked diminution in the total number of deaths in this State as compared with the equal period preceding it." He claimed the figures did not show it, and I could not find that they did.

THE PRESIDENT.— I certainly can answer for that, that the figures do show it.

DR. GAGE.— The total number of deaths in Massachusetts?

THE PRESIDENT.— Yes.

DR. GAGE.— He brought up his figures. I am very glad to be able to answer him now. I did not have the figures at hand.

DR. DURGIN.— The diminution in Boston alone has been from some 700 or 800 deaths a year down to less than 200.

THE PRESIDENT.— It seems to me that Boston has furnished in this matter what it has furnished in so many other matters of preventive medicine, results of the very highest importance. The south department of the city of Boston, in its hospital, which is by all odds the best hospital for the control and treatment of infectious diseases, communicable diseases, that I know of anywhere,— certainly, the very best in this country,— is under the active administration of a man who believes in what seemed to me absolutely unnecessarily high doses of antitoxin. It seems to me that Dr. McCollom uses antitoxin more recklessly than any human being on the face of the earth. I say that because the State furnishes it. But there is no evidence whatever in the records of the hospital that I have ever been able to find that that use has been mischievous, injurious, to a single patient. The record is a wonderfully good one. In talking with Dr. McCollom on the subject, I criticise, as I say, this enormous use of it; and yet when he says to me, "I know perfectly well that I differ from any other authority perhaps in the world in this matter, but, then, my patients get well," there is absolutely no answer to that.

DR. CHAPIN (of Springfield).— How much does he give, Doctor?

THE PRESIDENT.— Oh, two, five thousand units without any hesitation,— twenty-five thousand in one case.

Is there anything else to be said upon this subject? It seems to me the afternoon has been a singularly productive one and a singu-

larly interesting one. I think we have to thank Dr. Hill again, as we did Dr. Richardson, for his contribution. Is there anything else to come before the Association at this time? If not, it will be quite proper that we adjourn, I presume, if any one will make a motion to that effect.

A motion to adjourn was made.

THE PRESIDENT.— Dr. Durgin makes a motion that we adjourn, to meet at Gallop's Island in July. If that be your pleasure, you will signify it by saying, Aye: contrary-minded, No; and it is a vote.

Adjourned.

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